SCIENCE.

FRIDAY, JUNE 12, 1885.

COMMENT AND CRITICISM.

LIEUT. CORNWELL, who is to carry on the series of latitude observations at the U.S. naval observatory, referred to in a previous number of *Science* (vol. v. p. 60), has recently returned from an interview with Professor Oom, the director of the observatory at Lisbon. A list of eleven stars has been selected, and the details of the work have been agreed upon. But two stars will be observed in a night, each star being observed east and west of the meridian before the succeeding star is taken up. Some fifteen or twenty observations of each star will be made during the year. With the exception of *a* Lyrae, the stars range from the fourth to the sixth magnitude, and the greatest zenith distance at which any star will be observed at Washington is not greater than twelve or thirteen degrees. It is proposed to erect an azimuth mark for testing the stability of the instrument; and a careful determination of the level will, of course, be made with every observation.

This question of the variability of latitudes is one of considerable interest. Theoretically, periodical changes of latitude may occur, and an examination of observations made at a number of northern observatories during the past seventy-five years — Königsberg, Milan, Naples, Paris, Pulkowa, and Washington — appears to confirm the existence of such changes. At Pulkowa, which furnishes the most careful series of observations, a diminution of the latitude of $0.23''$, equivalent to about twenty-three feet, is indicated between the years 1843 and 1872; but in all these cases the variations are small, and we must be extremely cautious in ascribing them to actual changes of latitude. A series of observations made at Willet's Point by young engineer officers, under the direction of Gen. Abbot, also appears to have some interest and possible bearing on the question. By these observations a diminution of ninety-five feet is shown in the latitude since 1880; but here, again, it is quite possible that this apparent change may be due to errors of observation. Fergola's plan of making a careful series of observations at pairs of observatories, in about the same latitude, but differing considerably in longitude, will, if thoroughly carried out now, go far towards enabling us to give a definite answer to the question fifty years or more hence.

THE *American engineer* of May 8 contains an article on the levee system of river improvement which demands notice from all who wish the line between fact and fiction to be sharply drawn. The case presented is briefly this: the Mississippi-River commission, in 1883, asserted that they had restored the levees along the Yazoo front so as to exclude overflow from the head of that basin, and that as a consequence the height of flood at Vicksburg was about five feet lower in 1883 than it had been in 1882; and upon this assertion of facts the commission based an argument for the general construction of levees along the Mississippi as a means of channel improvement. The flood of 1884 came, and rose three inches higher at Vicksburg than it did in 1882, thereby completely overthrowing all the argument of the commission.

In the article referred to, 'J. B. J.' quotes from later reports of the commission proof that the commission had not restored the levees along the front in question in 1883, nor had it been done in 1884. Unless the clearly implied charge that this misstatement was wilful is successfully met by the members of the commission who signed the report of 1883 (General Comstock did not), it would seem that the

president of the United States ought to make inquiry, and relieve the country of the discredit which must come from the challenged veracity of an official body whose acts and sayings are being closely followed abroad and at home.

LETTERS TO THE EDITOR.

**** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Professor Hastings's theory of the corona.

I SHOULD be glad, with your permission, to make a few remarks with reference to a passage in Professor Hastings's letter in your issue of April 24. Professor Hastings states that he shows, in his report of the eclipse expedition to Caroline Island, that all the characteristics of the corona may be explained naturally and easily by his diffraction theory, *with the exception of the occasional filamentous structure.* The words which I have italicized convince me that Professor Hastings cannot have paid sufficient attention to the abundant and irrefragable evidence as to the solar corona which is afforded by photographs taken during total solar eclipses. These photographs prove that what Professor Hastings summarily characterizes as 'occasional filamentous structure,' constitutes the greater portion of the corona. In the photographs of the eclipse of 1871, there were more than a hundred distinct details of this kind, which I measured and drew, when assisting Mr. Ranyard in describing and cataloguing the details of the structure of the corona (*Mem. roy. astron. soc.*, xli. 657-686). These details were, of course, not all visible on a cursory inspection of the negatives; many of them were not perceived till after long study: but, once seen, there was no mistake as to their existence, and none were described that were not visible on at least three of the plates.

Moreover, since the coronal rays are very various in direction, and are seen in the negatives one behind the other, and at all angles of projection, it is evident that the corona must in reality be far more 'filamentous' than it appears in the photographs. To a greater or less extent, the same character is shown in negatives of other eclipses, though somewhat less of it is visible in some of the more recent photographs, probably on account of the greater density of the film in the case of those taken on the extremely sensitive dry plates.

I cannot enter into the optical points connected with Professor Hastings's theory, but simply wish to point out, that, if it will account for every thing except the 'filamentous structure,' it accounts, after all, for very little. W. H. WESLEY.

Royal astron. society,
Burlington House, London.

The natural gas-wells of north-western Ohio.

The gas-wells that have been drilled within the last year in Hancock and Wood counties, O., have furnished some interesting, and to some degree unexpected, information as to the geological foundations of the state. They show the presence of several formations that nowhere appear in outcrop within the limits of Ohio. The section furnished by them agrees quite closely, as to its elements and its general lithology, with the New-York scale.

I have lately examined the carefully kept records and drillings of six of these wells. They agree entirely in their main features. All begin in upper Silurian limestone, and all find their main supply of gas in the Trenton limestone. The section furnished by them is as follows:—

	Feet.
Niagara limestone, gray and blue, dolomitic	200
Niagara clay, a characteristic bed in central Ohio	2-4
Clinton limestone and shale, high colored	75
Medina shale, red and blue	50-100
Hudson River shale, gray and blue	400-500
Utica shale, dark, almost black, in places	275
Trenton limestone	300
Bird's-eye limestone	?

The Trenton limestone was drilled through in but a single well.

The Niagara clay contains characteristic fossils, as does also the Hudson-River shale and the Utica shale. The former shows chaetetoid corals, and fragments of Zygospira and Orthis. The Utica shale contains Leptololus insignis Hall, and fragments of the spines of Echinognathus of Walcott apparently. The Trenton limestone is crystalline and hard, but it shows the presence of fossils in abundance.

The gas obtained from the wells is delivered with moderate pressure. It contains a notable quantity of sulphuretted hydrogen. It is used so far mainly for heating and for steam-production. Judicious estimates put the amount yielded each day by three wells in Findlay, the county-seat of Hancock county, at five hundred thousand feet. EDWARD ORTON.

Columbus, O., June 1.

A tropical American turtle on Anticosti.

Professor John Macoun, botanist to the Canadian geological and natural-history survey, has shown me a turtle which was given him by the light-keeper at West Point, Anticosti, in August, 1883. It was found living near the lighthouse, and was the only one seen by the keeper during his twenty years' residence on the island. Mr. F. W. True, to whom I sent the specimen for identification, pronounces it to be a half-grown Chelanoides tabulata (Walbaum) Agassiz. The habitat of the species is tropical South America and the West Indies, whence it was probably brought to Anticosti on some vessel. C. HART MERRIAM.

Abert's squirrel.

On the 10th of April last, on my return from a five-days' visit to the pueblo of the Zuñis in New Mexico, I drove through an extensive pine-forest, which the road enters a few miles from Fort Wingate, my destination.

There were in the ambulance with me, besides the driver, Prof. J. W. P. Jenks of Brown university, curator of its museum, and a fellow-traveller, a friend from Philadelphia. Professor Jenks was eagerly on the lookout for rare things in south-western birds and mammals for his college museum, while his friend was enjoying himself in examining two specimens we had taken along the road, and joining in the conversation as best a layman may, when two enthusiastic naturalists formed the odds against him.

Suddenly the driver stopped the conveyance, and directed my attention to a large gray squirrel that had just scampered up the trunk of one of the lofty pines, and was now sitting, partly hiding, on the lower limb, close to the body of the tree.

In a moment this magnificent creature was mine, dead at my feet.

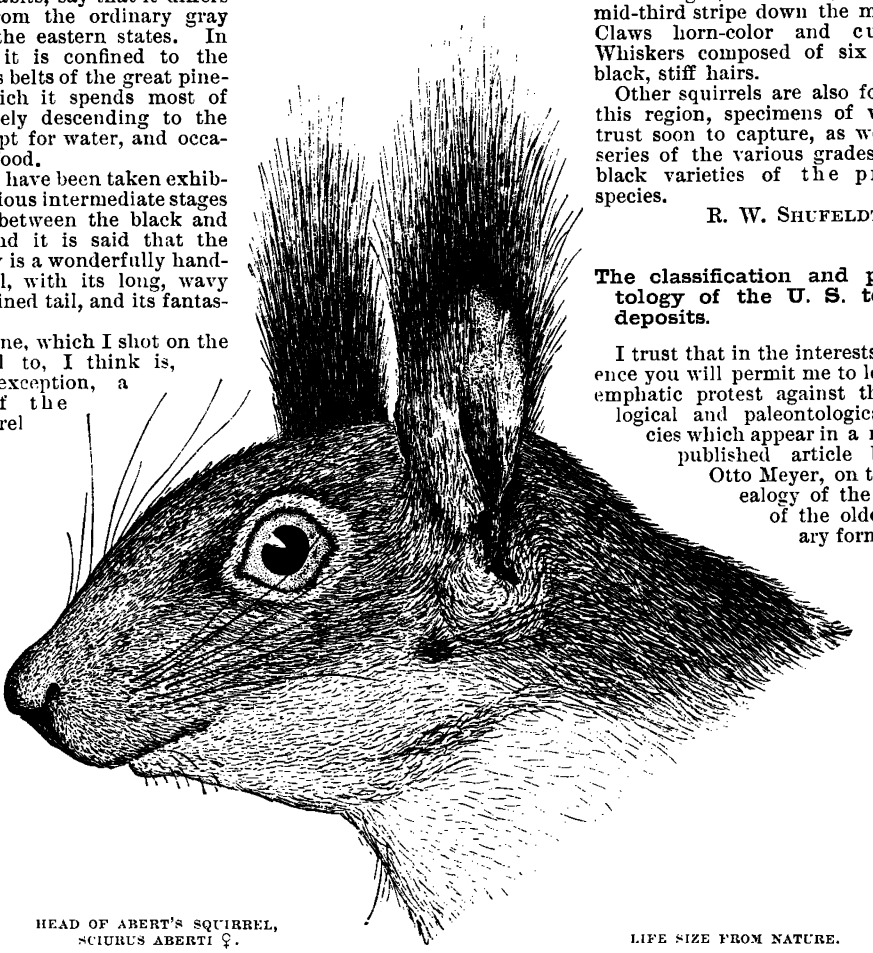
It proved to be a fine female specimen of Abert's squirrel in the gray pelage; and I subsequently learned from others who have hunted them in this locality, where they are by no means abundant, that they are sometimes taken where their fur is of a jetty black, with the tail broadly bordered with snowy white, and perhaps similarly marked on the breast and lower parts.

Old hunters who have had the opportunity of observing its habits, say that it differs but little from the ordinary gray squirrel of the eastern states. In this region it is confined to the mountainous belts of the great pine-trees, in which it spends most of its time, rarely descending to the ground except for water, and occasionally for food.

Specimens have been taken exhibiting the various intermediate stages of coloring between the black and the gray; and it is said that the black variety is a wonderfully handsome animal, with its long, wavy white emargined tail, and its fantastic ear-tufts.

The gray one, which I shot on the day referred to, I think is, without exception, a specimen of the finest squirrel in the fauna of our country. I have no acquaintance with another American species that can compare with it.

I do not remember having seen at any time a drawing of this squirrel: so, after having carefully prepared the skin and skeleton of my specimen, I made the life-size figure of its head which illustrates this letter.



HEAD OF ABERT'S SQUIRREL, SCIURUS ABERTI ♀. LIFE SIZE FROM NATURE.

From it I also took the following measurements, and description of its external characters and appearance:—

	Centimetres.
Length of body from tip of nose to root of tail, down the back	35.0
Length of tail	34.0
Height of ear, including tuft	6.5
Fore-paw, from inner pad to longest claw	4.3
Hind-paw, from inner pad to longest claw	4.8
Tip of nose to anterior canthus of eye	3.2

Entire upper parts of a grizzly, iron gray. Lower halves of inner aspects of ear-tufts, and a median broad stripe from shoulders to near root of tail, of a brilliant chestnut. Ear-tufts large, composed of straight black hairs. Entire under parts, borders of tail, circum-ocular stripe, and upper sides of feet, pure white. A rather broad dividing-line at either side, between the white of under parts and gray above, jetty black. Central hairs of tail, for its entire length, also black, forming a mid-third stripe down the member. Claws horn-color and curved. Whiskers composed of six or ten black, stiff hairs.

Other squirrels are also found in this region, specimens of which I trust soon to capture, as well as a series of the various grades of the black varieties of the present species.

R. W. SHUFELDT, M.D.

The classification and paleontology of the U. S. tertiary deposits.

I trust that in the interests of science you will permit me to lodge an emphatic protest against the geological and paleontological fancies which appear in a recently published article by Dr. Otto Meyer, on the genealogy of the species of the older tertiary formations.

The article displays such a monstrous disregard or ignorance (or both) of the literature of the subject of which it treats, and so fully betrays the author's misconception of the numerous species that have been described from the region in question, that it would not even call for a protest, were it not for the air of respectability which is given to it by the cover of the *American journal of science*.

Little can and need be said in response to a thesis which maintains that there is not sufficient evidence to prove that the Vicksburg beds overlie the Claiborne sands, and that, as a matter of fact, the latter will be found overlying the former, when not a particle of evidence is brought forward in support of this statement.

I take this opportunity, also, of warning paleontologists against the acceptance of the numerous new species, which, without either description or proper comparison, are claimed by Dr. Meyer.

ANGELO HEILPRIN.

Academy of natural sciences,
Philadelphia, June 3.

Premature appearance of the periodical cicada.

On the morning of Oct. 12, 1884, when I chanced to be in Virginia, near Clifton station on the Midland railroad, my attention was attracted by hearing at some distance the characteristic, and to me perfectly familiar, note of the periodical cicada (C. septendecim). Regarding this as a somewhat novel occurrence at that time, I decided to investigate it, and at once proceeded in the direction from which the sound emanated. Though the notes were, as usual, interrupted by short intervals, I found it easy to correct my direction with each recurrence of the sound, and was soon at the foot of some small oaks in which the insects were located. There were at least three males, and the interval between the notes was quite short. I stationed myself under one of the trees, and carefully located the spot from which the sound of one of the insects proceeded. Although it was not possible, from any position I could assume, to see the insect itself, hidden as it was in the dense foliage, and at the height of some twenty feet, yet I soon knew within a few square feet the precise part of the tree occupied by it. I remained some fifteen minutes listening to the peculiar murr-r-r-r-r-row with which I had been deeply impressed when a boy (1854 or 1855) in my native state (Illinois) at the time of the great swarm that left its withering blight on all the vegetation, but which I have since heard for days together as late as 1878. I think all who are really familiar with this sound will agree with me that it has no counterpart in the whole range of sound-producing creatures. The body of the note lasts, on an average, about two seconds, upon a uniform key, when, without being interrupted, the pitch rapidly drops, with what musicians call a 'slur,' for, as near as I can judge, a full octave or more, and the note abruptly terminates. This peculiar termination is difficult to detect where the trees are full of the singing insects, but it is always present; and in this case it was clearly marked, affording me a fine opportunity for studying the phases of the note, and timing its length. Had I been an entomologist, and aware how anomalous this occurrence was, I should doubtless have persisted until I had secured a specimen, and should have searched for exuviae, etc.; but as I felt absolutely certain as to what I heard, and did not know but that it might be a somewhat ordinary occurrence, I merely made a note of the facts, and leisurely left the spot.

Several days afterwards, happening to be in conversation with Prof. C. V. Riley, I casually mentioned the circumstance as a fact in his line, fully expecting him to reply that it was no very unusual thing. To my great surprise, he pronounced it impossible, and wholly discredited the accuracy of my observation. He said I must have heard some other species of cicada; and, when I asked him what other species had a note precisely like that of the periodical one, he could do no better than to name the common harvest-fly (Cicada pruinosa), the sharp, shrill note of which was also perfectly familiar to me, and so different that I could no more confound it with the other than I could the chirp of a sparrow with the cooing of a dove. My attempts to convince him by describing the sound were as ineffective as though I had been speaking to one who was himself unfamiliar with it.

Having the courage of my convictions, I made bold, on the first opportunity, to lay the subject before a Washington scientific body in the form of a verbal statement of the case, whereupon the learned professor surprised me, not only by no longer positively gainsaying it, but by propounding a theory according to which he admitted the possibility of my observation having been correct. His theory was, that, owing to the exceptional heat of the latter part of that season, a few of the brood of 1885 which were nearest the surface might have been prematurely brought out the autumn before. This seemed very reasonable to me, and I promptly (and seriously) congratulated Professor Riley on having discovered a theory to explain my fact.

Here I supposed the matter was to rest; and here it did rest until a few days ago, when to my further surprise, at the close of an exceedingly interesting paper which Professor Riley read before the same society, on the brood of cicadas which has just appeared, he took occasion to bring up the subject of my Virginia observation, and to pronounce it utterly worthless, and the occurrence impossible as contrary to all the canons of entomology. On being reminded of his own theory, above stated, which he seemed to have forgotten, he could not disclaim it, and virtually renewed it, leaving himself in the position of both denying and admitting the possibility of the event.

I do not make these statements with a view to arousing a controversy, but solely in the hope that some of your many observant readers may be able to confirm and perfect the confessedly incomplete record which I hereby make of this singular incident.

I will, however, venture a suggestion drawn from a field with which I am better acquainted. The theory of Professor Riley might, I think, be greatly strengthened by facts derived from plants. The effect of a protracted warm spell in autumn upon the vegetation of this climate has been the subject of investigation on my part for a series of years; and the autumnal flowering of strictly vernal species is a fact attested by a score or more of species, most of which have been recorded and published. It is not contrary to the canons of botany, but consonant to a rational understanding of causes and effects. And why should not similar causes produce similar effects on insects? For one, I cannot doubt that they do so; and I am as firmly convinced now, as I was at the time, that the sound I heard proceeded from veritable seventeen-year locusts that were thus prematurely brought from their long subterranean dungeons into the genial sunlight of that warm October day.

LESTER F. WARD.

Washington, June 6.

The recent Chicago storm and the sun-glow.

The telegraph reports a very violent thunder-storm at Chicago during the night of June 2; the lightning striking many buildings, and causing the loss of five lives. I was in Chicago during a part of Monday, June 1. At that time the reddish glow around the sun which I have recently described in your columns, was almost as intense as I have ever seen it even in Colorado. In Colorado any great increase in the depth of tint of the circumsolar glow portends a fall in temperature with conditions favorable for cold electrical storms. The rule would seem to be about the same at Chicago, though the Great Lakes may tend to prevent the formation of hail near them.

G. H. STONE.

Portland, Me.

VEGETABLE MORPHOLOGY A CENTURY AGO. — LINNÉ AND WOLFF.

"Um die geschichte der wissenschaften aufzuklären, um den gang derselben genau kennen zu lernen, pflegt man sich sorgfältig nach ihren ersten anfängen zu erkundigen." — GOETHE.

In order to clear up the history of the sciences, and to learn to know with exactness the progress of the same, we are wont to give careful attention to their earliest beginnings.

To students of natural science no term is more familiar than 'morphology,' and no doctrine more commonly accepted and understood. Pre-eminently reasonable and natural, the morphology of plants in particular appeals to the simplest understanding. Every schoolboy who has prepared his perfunctory herbarium, or accomplished his stint in plant-analysis, knows something of the meaning of a flower, can tell something of its natural history, — how that bract and sepal and petal, stamen and carpel, are but so many modifications of an ideal leaf, so many varied expressions of a single thought. Likewise the facts to be cited in proof of such assertions are familiar to every-day experience. Who has not gathered pond-lilies, and noted how, by the steps of imperceptible transition, Nature passes on from green sepal to perfected anther? 'Double' flowers of all sorts grow in country gardens, and in springtime the woodland offers anemones which are both 'double' and green. Even prolification is widely known in fact, if not in name.

To all these morphological facts, strange and curious as they certainly are, no one ever attempts to apply any other than the accepted explanation: no other is conceivable, none other is needed. And yet much of the ease with which such explanations are received must be considered due to the habits of thought now prevalent in the world, to the very atmosphere in which to-day men are called to think, to judge. In all the world of thought, ideas of transition are so rife, that unity or community of origin, even of objects most dissimilar, excites small surprise: it is the natural supposition. A different atmosphere, different habits of thought among men, would change completely the simplicity of many a modern page. It is, then, not surprising that a century ago morphology, as we know it, had not so much as found a name; that, with the same facts before them, the best minds in Europe were struggling to the perception of this simple theory, which the schoolboy may now appreciate and understand. The first perception of natural truth, like the opening-up of unknown lands, is a discovery, dim enough when seen in prospect, however easy when once accomplished. Linked with the botanical discovery here to be considered are three most brilliant names, two the brightest of their century, — Linné, Wolff, Goethe; not that all contributed equally to the establishment of the truth, but that to each the problem came, and for it each found answer. What answer to the floral problem each of these great men could give, it is our purpose here briefly to set forth, considering first the labors of Linné and Wolff, later those of Goethe.

Linné, the first in order of time, may be said to have discovered the problem. Passing his life in the study of flowers, the question 'What is a flower?' must have come to the great botanist again and again, pressing him by its very omnipresence almost to his annoyance. But to Linné, fortune-favored, the whole natural world lay like an undiscovered country, — a world too wide for the comprehension of any one mind, however active or versatile. It has been the marvel of all men since his time, that Linné did so much, that his instincts were so true, that to so many questions he gave answers which are the end of controversy. But as regards the morphological problem, the great naturalist seems never to have arrived at a definite conviction. Every thing he says on the subject is more or less obscure. Here, for once, he seems to have reasoned *a priori*, and fancy strangely supplements and distorts the facts discussed. The coincidence of number afforded by the successive layers in the make-up of the stem and the successive circles of organs in the composition of the flower struck him as affording a plausible explanation of the origin of the latter structure. Here are four layers, — the outer bark, the inner bark, the wood, and the pith. The outer bark is often on growing stems green, passing to all appearances imperceptibly into the green covering of the calyx; from the inner bark, white and delicate, come the delicate petals of the corolla; while from the cellular xylem and pith of the stem arise the circles of stamens and carpels respectively; and in the young flower-bud are not the organs last named simply masses of cellular tissue hardly to be distinguished from forming wood and pith? A more careful anatomy would have revealed the mistake; for, as Goethe points out in this connection, "it is the inner bark alone which possesses all power of life and growth;" the other parts of the stem having in the main taken on definite character, and been relegated to inactivity. If we may regard the 'pith' at the end of the growing axis as primary *meristem*, then so far so good; and the fancied relationship is not without its grain of truth.

But Linne did better than this toward the solution of our problem. In his 'Philosophia botanica' of 1751, he, among other things, makes the following propositions: —

"Principium florum et foliorum idem est,"
"Principium gemmarum et foliorum idem est,"

which, so far as it goes, would seem a clear statement of the truth; but it is doubtful whether the author, as he wrote, appreciated the full import of his words. Certainly his immediate followers and pupils did not. He stood face to face with the truth, but recognized it not, and turned away from it, and from the only line of thought which could possibly lead to light, only henceforth to wander in vain speculations and obscurities pertaining to his theory of prolepsis, — a theory understood neither by his contemporaries, his successors, nor possibly even by himself.

But while Linné was thus hopelessly lost in the mazes of his own imaginings, another mind, working in an entirely different field, took cognizance of the problem. A young student, afterwards known to fame as Caspar F. Wolff, away in central Germany in Frederick's university of Halle, had caught the spirit of genuine scientific research, and in his thesis for graduation in 1759 published an exact, succinct, and perfectly clear statement of the modern doctrine of vegetable morphology. Wolff had ideas of his own concerning generation in all the organic world, more particularly in the world of animal life. His taste lay in the line of anatomy in its ordinary scope; and the reference in his thesis to matters botanical was entirely apart from the chief purpose of his dissertation, simply incidental for the sake of completeness; and perhaps, with the propositions of Linné, above cited, before him, he had no thought of propounding any thing new to botanical science. In perfect harmony with his subject, Wolff undertook to elucidate the origin of the various organs of a plant, and in so doing was struck with the extraordinary similarity everywhere patent. Regarding the involucre of the 'compound' flower as calyx, he perceived easily the intergradation of foliage and sepals; the ripened capsule, with bursting sides, afforded evidence of the foliar nature of the carpels; that the seed is largely made up of leaves, appears when it germinates, and the cotyledons assume and perform, to some extent at least, the leaf's function; sepals and petals are often interconvertible, and stamens not infrequently show transition to petals: consequently in the entire plant, so far as immediate analysis goes, we find nothing but root, stem, and leaves.

As Wolff's thesis had to do with generation, and not at all with botany, it is a matter of no surprise that he regarded all this simply as introduction, and went on with his 'theoria generationis,' alleging that the formation of flower and fruit is due to failing energy in the plant; that all modifications have origin in the gradual withdrawal of vegetative power, which diminishes in amount as growth continues, and finally vanishes altogether. What Wolff hoped might be science, has been forgotten; what he lightly esteemed, is science, — fact not without significance, and certainly not without parallel in the history of intellectual work.

But if Wolff did not appreciate what he had accomplished, neither did any of his contemporaries. The seed fell not into good ground. The great Haller was yet living and working, at once botanist, anatomist, and poet; but he saw not the truth, although certainly familiar with Wolff's writings. The Jussieus were busy in Paris, arranging and re-arranging in the Jardin des plantes; but they heard nothing of Wolff: the time was not yet. The scientific vision of the age, dazzled by a sudden discovery of Nature's richness and variety, was not yet ready to be concentrated upon any single problem, however interesting that problem might be in statement, or far-reaching in outcome and solution. T. H. McBride.

VELOCITY AND SEDIMENT.

The observations on velocity and sediment on the Mississippi River, from Cairo to the head of the Passes (1,060 miles), have not confirmed the conclusion of Mr. Login, in 'The benefits of irrigation in India,' regarding the relation between these two functions of flowing water. His conclusion is thus stated: "The author believes that the power of water to hold matter in suspension is directly as the velocity, and inversely as the depth. It is also suggested that water in motion rolls rather than slides, and that it is owing to this rotary motion that water has the power to hold matter in suspension; further, that, with given velocities and defined depths, only a certain quantity of matter can be held in suspension, whatever may be the character of the bed or bank of the river or canal. If the velocity be increased, and the depth remain constant, scour will take place. If the velocity be decreased, and the depth is the same, there will be deposit."

The suggestion 'that water in motion rolls rather than slides,' is valuable as explaining the 'inner movements of the particles of water among themselves,' which are aggregated in the indirect currents found even in carefully prepared beds, which are symmetrical, smooth, and straight. These movements were well illustrated by Mr. Francis, in a series of experiments at Lowell in 1867, by mixing whitewash with the clear water of an artificial channel. But the movements of mass inaugurated by the relative movements of the particles of water among themselves appear too feeble to account for the immense quantities of sediment observed in suspension in great alluvial rivers. It also seems clear, as concluded by Mr. Herschel, in his paper on the erosive and abrading power of water, "that direct friction tends to drag materials along the bed, or down the banks, if these have a sufficiently steep side-slope," and that "the effect of the simple friction of a stream upon its bed and banks is not a source of danger: its action is very slow, and it has never been shown to be of a dangerous character in any instance." Certainly the sedimentary grains have no power of motion independent of the water surrounding them; and friction against a smooth bed could not impart such vertical movement to the water as is necessary to lift these grains, except in the slight degree that may result from the movement of the particles of water among themselves. The conditions that prevail in natural stream-beds are necessary for a great suspension of sediment. The projections, inequalities, and sinuosity of such beds expose the material composing them to the impact of the current rather than to simple friction, and also cause those extreme indirect movements of large masses of water that, in great rivers, develop whirls, boils, and eddies, and which alone are capable of lifting numerous and coarse grains of silt, sand, and even gravel. When a 'boil' rises in the Mississippi River, the surface may be raised many inches, and the charge of sediment so dense, that it is seen to roll away from the crater in cloud-shaped masses.

The suspension of sediment is, then, only an indirect result of the velocity, depending more directly upon the character of the bed — its symmetry, smoothness, and straightness, — than upon the velocity, or the relative depth of its different reaches and stages.

The amount of sediment suspended throughout the Mississippi River appears, however, more controlled by the tributary most largely supplying its volume at the time than upon any or all other causes. It was noticed by Capt. Brown, U. S. corps of engineers, that "the Missouri was one of the greatest contributors of sand to the Mississippi River," observably even in the South Pass, 1,300 miles below its mouth.

The apparent anomalies in the following table are largely explained by the relative discharge of the Missouri and the clear-water tributaries. It will be observed that frequently the aggregate bulk or weight of sediment passing per second (2 × sediment in each foot) is greater at or below a mean stage than it is at the maximum.

In this table the velocity is divided into half-feet per second; and in the other columns are given the number of periods during the series of observation in which each rate prevailed, the number of separate measurements taken in each period, and the mean quantity in each period at each rate of velocity.

Fulton, Nov. 27, 1879, to Oct. 12, 1880, Mississippi River commission.

Velocity, feet per second.	Periods at each rate.	No. of observations.	Mean mgr. of sediment in 500 c.c.	Velocity, feet per second.	Periods at each rate.	No. of observations.	Mean mgr. of sediment in 500 c.c.
2.0 @ 2.5	1st	11	339	5.5 @ 6.0	1st	1	235
	2d	14	361		2d	1	530
2.5 @ 3.0	1st	9	511		3d	8	536
	2d	4	311		4th	1	355
	3d	5	434		5th	2	915
3.0 @ 3.5	1st	4	311		6th	3	813
	2d	6	647	6.0 @ 6.5	1st	1	335
	3d	9	524		2d	1	200
3.5 @ 4.0	1st	1	290		3d	6	317
	2d	8	271		4th	1	380
	3d	4	754		5th	1	275
4.0 @ 4.5	1st	3	215		6th	5	1,062
	2d	4	386		7th	1	840
	3d	4	281	6.5 @ 7.0	1st	1	410
	4th	12	538		2d	1	260
	5th	2	825		3d	1	180
4.5 @ 5.0	1st	2	240		4th	4	600
	2d	2	460		5th	1	1,085
	3d	4	439	7.0 @ 7.5	1st	3	400
	4th	5	777		2d	3	562
	5th	3	758		3d	2	140
5.0 @ 5.5	1st	1	390	7.5 @ 8.0	1st	1	325
	2d	2	885		2d	2	145
	3d	3	908				

From the theory of Mr. Login, before quoted, the inference is drawn, that when flowing water is saturated, or loaded in proportion to its velocity with sediment, the erosion and caving of banks will cease. In this is involved the assumption that the erosive power is that of friction rather than of impact. Surveys have been made by the Mississippi River commission which show the relative amount of caving on the right and left banks below Cairo. In this part of the river the line of demarca-

tion between the muddy discharge of the Mississippi, following the right or west bank, and the clear water from the Ohio along the left or east bank, is apparent at times to the eye for twenty or thirty miles, and to the sediment trap for ten times this distance. At Columbus, twenty miles below Cairo, the amount of sediment per unit of measure has been observed three or four times greater on the west than on the east side of the river. But these comparative surveys show that the caving on the right or west bank, washed by the muddy water, is greater, both in length of bank and in area and bulk, than it is on the opposite shore, where the water is undercharged with sediment. The length of river from which this conclusion is drawn (230 miles) is thought to be great enough to eliminate any local or abnormal influences on velocity, or material of bank.

B. M. HARROD.

AMERICAN CLIMATOLOGICAL ASSOCIATION.

THE second annual session of the American climatological association was held at the hall of the Academy of medicine in New York, May 27 and 28; the president, Dr. A. L. Loomis, in the chair. This association was organized a year ago in Washington, for the study of climatology and diseases of the respiratory organs.

In the opening address, on the afternoon of the 27th, the president expressed the opinion that the scope of the society's work ought to be enlarged, so as to include the study of affections of the vascular system and other diseases, as well as to investigate more systematically the subjects of sanitaria and of mineral springs. This suggestion was afterwards adopted.

In Europe the study of these subjects had gone much farther than in America, and it was becoming more and more common for European physicians to treat their patients by prescribing residence for greater or less time at sanitaria especially adapted to their diseases. That such was not the case in America was not due to the lack in this country of the climates which possessed the necessary beneficial qualities, but to the lack of systematic study of the subject by the medical profession, as well as to the imperfect provision for the wants of invalids at our health resorts. The attempt to remedy these deficiencies was one of the main objects of the association.

As a model of what was desirable to encourage in the way of sanitaria, the president described one in the Adirondacks, devised to meet the wants of invalids of limited means, where, for a small sum, accommodation is provided in cottages for two or four patients, with a common dining-hall; also tents, etc., for those who are able to camp out; the whole under the charge of a medical board and an attending physician.

After speaking of the causes of pulmonary phthisis, Dr. Loomis said that the objects to be accomplished in the treatment of the disease are two, — to improve the general condition of the patient; and to stop the local disorder in the lungs, preventing the entrance and multiplication of the bacilli tuberculosis. Good climate is a potent means of accomplishing both these ends. Good climate means pure air, and it must be determined by clinical experience.

The amount of moisture in the atmosphere is not an index; for the only dampness injurious to the phthisical patient is that exhaled from the soil, laden as it is with organic matter. The nature of the soil is therefore of prime importance. It must be light and porous, admitting of good drainage. A clayey soil is necessarily bad.

Dr. Beverly Robinson of New York read a paper on antiseptic inhalations, instancing cases in which he had obtained decided benefit, especially in the way of alleviating cough by this method of treatment. The vapor of antiseptics was applied by means of a respirator worn over the nose or mouth, or both, from one to two hours a day in some cases, and nearly all the time in others.

'Catarrhal affections of the nasal cavities as a cause of pulmonary phthisis,' was the title of a paper by Dr. W. C. Jarvis of New York. The position taken was, that consumption can be traced, in many instances, to a catarrhal condition of the larynx, which in its turn is induced by the irritating effect of the discharges from the pharynx and nasal cavities, the catarrhal condition in this situation being due to a deflected nasal septum. In the discussion which followed, it became evident that the members present were not in accordance with Dr. Jarvis's novel views.

The event of the evening session was the presentation, by Dr. H. F. Williams of Brooklyn, of his pneumatic cabinet, with histories of cases of consumption treated therewith. The cabinet consists of an air-tight iron safe, with a plate-glass front, perforated near the middle with an opening about an inch in diameter, through which passes a rubber tube, so arranged that the patient can hold the end of it in his mouth as he sits upon a low stool in the cabinet. When the cabinet is closed, this tube forms the only means of communication with the external atmosphere. The patient having taken his seat, and placed the end of the tube in his mouth, the door of the cabinet is closed, and the air within slightly rarified by means of a few strokes of an air-pump. By this process the body is surrounded by a rarified atmosphere, while the respiratory passages are in communication with the outer air: the patient is therefore breathing from an atmosphere of greater density than that surrounding his body, with the effect of expanding the chest, bringing the lower part of the respiratory organs into play, and affording valuable exercise of muscles of respiration. Dr. Williams also claims that with its aid antiseptic sprays and vapors can be carried much deeper into the lungs than by any other method. The paper and demonstrations excited much interest, and a number of gentlemen expressed them-

selves as deeply impressed with its value as a therapeutic agent.

Dr. F. C. Shattuck followed with an exceedingly interesting and scholarly paper upon the home treatment of consumption.

The morning session on the 28th was opened by a paper from Dr. Ch. Dennison of Denver, Col., upon a rule for the even division of climate. The ground was taken that dryness and elevation are the most important elements of climate in the treatment of consumption.

'The problem of acclimatization' was the title of a paper read by Dr. I. H. Platt of Brooklyn. The question of acclimatization, although a long-vexed one, had never been satisfactorily dealt with, partly, no doubt, because most of the discussion antedated the advances of recent years in the sciences of biology and anthropology, especially as developed by the great principle of evolution. Facts were adduced to show the perfect adaptation of races to climates widely differing from their own, and which are at first injurious to them. The acclimatization of the Spanish race in Peru, and of the French in Algiers, were cited as examples. The whole subject was but an application of the fundamental law of biology enunciated by Herbert Spencer, — that of adaptation to environment. In the adaptation of a race to new surroundings, the principle of natural selection would play an important part. And the author took the ground, that, even in the acclimatization of an individual, the same law would find application, as the protoplasmic elements of the body would be subject to it in their growth and development. The author called attention to the exceedingly complicated nature of the problems presented by the action of the many elements of climate upon the human organism, and the modifications and reactions effected therein, and the importance of their more thorough and systematic study.

The public session in the afternoon was opened by Dr. J. C. Wilson of Philadelphia, with a paper upon the climate of Florida; and Dr. Keating, also of Philadelphia, followed with a paper upon the same subject. Both gentlemen took substantially similar views, Dr. Keating dwelling principally upon the climate of southern central Florida. This region is characterized by sandy soil, pine woods, and continual sunshine. The climate is more equable than that of the northern portion of the state. The counties of Orange and Orlando are the most favorable spots. There are good hotels, fine drives, and all the facilities for comfortable living. The summer as well as the winter climate is desirable. The doctor much preferred the central portion of the peninsula to either coast as a health resort.

Dr. E. Darwin Hudson, jun., treated of the results of the home treatment of consumption, contrasted with those of changed residence and climate, and presented cases in his own experience in which he had obtained favorable results where, for some reason, change of climate was inadmissible.

'Hay-fever and allied affections' was the title of the contribution of Dr. F. H. Bosworth of New York. A brief sketch was presented of the history of the various theories in regard to the causation of hay-fever, — first, that it is caused by the pollen of ragweed and other plants; second, Dr. Beard's theory of a nervous tendency, which he claimed was hereditary; third, the theory of Dr. Daily of Pittsburgh, that it was caused by hypertrophy of the nasal mucous membrane. Neither of these elements alone is capable of producing the disease: it is the effect of all three acting together; and the removal of any one is sufficient to cure the disease. The most practical and radical method, however, is by attacking the hypertrophied mucous membrane. The doctor explained the action of the hypertrophied membrane in causing the disease by its tendency to cause partial occlusion of the anterior nasal passages, in consequence of which the effort of inspiration produces a rarefaction of the air behind the partially occluded portion of the passage, thus, by a process analogous to dry-cupping, producing turgescence of the local blood-vessels.

Dr. D. M. Cammann closed the proceedings with a history of the stethoscope, and the presentation of a modification of the Cammann stethoscope devised by himself.

A reception was given in the evening by the New-York members to the non-resident members. The papers read before the association will be published in full at an early date in the *New-York medical journal*.

The following officers were elected for the coming year. President, Dr. William Pepper of Philadelphia. First vice-president, Dr. Frank Donaldson, Baltimore. Second vice-president, Dr. Beverly Robinson, New York. Secretary and treasurer, Dr. J. B. Walker of Philadelphia. Council, Dr. E. D. Hudson, jun., New York; Dr. E. T. Bruen, Philadelphia; Dr. J. H. Tindale, New York; Dr. J. C. Wilson, Philadelphia; Dr. F. H. Bosworth, New York.

CURVED BACILLI IN AIR AND WATER.

MR. J. HERICOURT (*Comptes rendus*, 1885, p. 1027) gives some interesting results of his investigations on the nature of curved bacilli, and their presence in the atmosphere. The researches were made during the recent cholera epidemic; and the following are his results: —

1°. In all water, no matter what its source (spring-water, from cisterns, running, or stagnant), there are curved bacilli of varying form and size, among which those of the same type as the cholera bacillus are constantly found.

Of the various waters examined, some were taken from localities absolutely free from cholera; others were examined very lately, when the disease no longer existed; and most of them were used for drinking-purposes, and were of perfect quality.

2°. The constant presence of these micro-organisms in all kinds of water can only be explained by supposing the existence of their germs in the air; and to test this, atmospheric dust was collected from dif-

ferent places, as from gardens, rooms, barracks, sick-chambers, stables, and out-houses. With this dust was sown neutral *bouillon* sterilized by heat, and cooked potato, with the result of finding many curved bacilli in all the cultures.

3°. The curved bacilli do not exist in their characteristic form in 'atmospheric dust:' they are there present in the germ or spore condition. In fact, if this 'dust' be examined immediately after dilution with sterilized distilled water, very few curved bacilli can be made out; and these are hardly recognizable as such, being changed in appearance by the development of one or more spores at their ends, or somewhere in the middle of the rod. This sort of change is precisely what is seen in cultures. If what happens in these drops of diluted dust be observed from day to day, the number of curved bacilli will be seen to vastly increase until the third or fourth day, when the spore formation recommences.

4°. The presence of curved bacilli in water, and of their spores in the air, furnishes a sufficient explanation of the presence of these organisms wherever air or water can penetrate.

Intestinal dejections in simple diarrhoea, as well as in dysentery and typhoid-fever, broncho-pulmonary secretions in all diseases of the lungs, from simple catarrh to advanced tuberculosis, pus exposed to the air, the saliva of a sick or well man, — all substances, in fact, which can nourish the germs of bacteria, contain the curved bacilli, and oftentimes in much greater number than the other bacteria, which are also found in such culture-media. The mud of the streets, made up of dust and water, can also be considered to be a favorable medium in which they are numerous and active.

5°. These micro-organisms are decidedly *aérobie*, and only flourish on the surface of liquids. They are mobile, moving with the rapid oscillations of vibrios, and very refractive. They are easily colored by methyl violet in watery solution, and, thus stained, show all the described forms, — commas, curves, omega, S, spirals, etc. In general, they are from one-half to two-thirds as long as the bacillus of tuberculosis, but are thicker and less regular than these: in fact, no peculiarity of form or staining distinguishes them from the bacilli found in cholera dejecta. Sowing the *bouillon* with dust proves that the spores, whose formation was observed as above, are their *resting* stage: moisture seems to be the condition indispensable to their perfect development.

6°. Collected first on *bouillon* or cooked potato, and then cultivated on nutrient gelatine, these curved bacilli form rounded colonies with serrated edges composed of highly refractive granules. These colonies, kept at 20°–22° C., grow in the gelatine, and liquefy it, finally producing a colony of the shape of a glove-finger.

7°. Until conclusive inoculation experiments shall be made, proving the pathogenic properties of the curved bacillus of cholera, the conclusion to be reached is, that these latter are the same as are found in all secretions, normal or pathological, provided these have come in contact with water, which is the normal habitat of curved bacilli, or with air, which furnishes transportation for the germs.

[These experiments are exceedingly interesting, but no proof is offered to show the exact correspondence of the curved bacilli spoken of, with those of cholera. Johne's work (*Science*, June 5, 1885) speaks of the distinctive difference between Koch's comma bacillus and that of Finkler and Prior; and this latter will answer to all of the description given by Hericourt of the curved bacilli he has observed.]

HYPODERMIC INJECTION OF CULTURES OF CURVED BACILLI.

IN a paper on the effects produced in man and animals by the ingestion and hypodermic injection of cultures of the bacteria of choleraic diarrhoea (*Comptes rendus*, 1885, 1148), are given some interesting results obtained by Bochefontaine in experiments made with cultures obtained from choleraic diarrhoea in peptonized gelatine.

The first generations were found to liquefy the gelatine with a cup-shaped depression terminated by a deep point. None of the cultures contained the curved bacillus alone; but always, and in greater number than this, were found rods or spirilla fully developed. There were never found in the cultures the very short, rapidly moving bacteria which filled the watery discharges in cholera. Every successive generation showed an increase in the number of the simple curved bacilli.

I. The author has on four different occasions swallowed pure cultures of the curved bacillus of the third and fourth generation without ill effect.

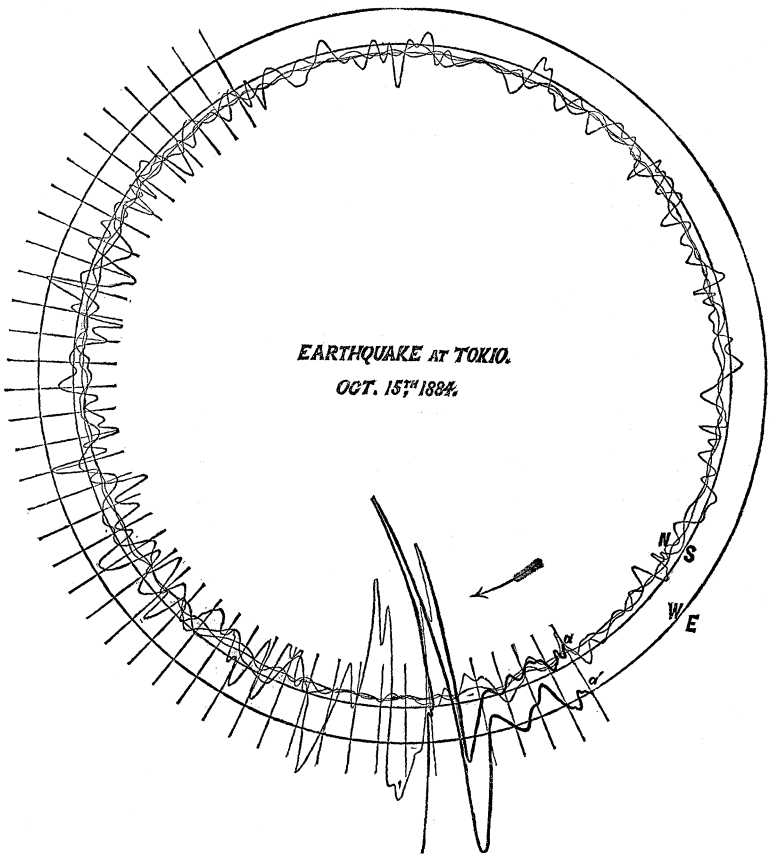
II. Two adult guinea-pigs were inoculated in the flank with a fourth of a centimetre of a mixture of equal parts of water and gelatine containing the culture: both were found dead the next morning. The autopsy showed great effusion on the inoculated side and opposite abdominal wall, with nothing in the internal organs. Two other guinea-pigs were inoculated with an eighth of a centimetre of the same mixture, and the smaller one died in twenty-four hours, with appearances similar to the first two. The second showed no symptoms. Microscopic examination of the blood of the three dead animals showed nothing. The same injection was made in two larger guinea-pigs, with no result.

III. The experimenter injected three-fourths of a centimetre of the mixture under the skin of his left fore-arm, with the result of much oedematous swelling and some pain, with deep fluctuation in the region of the puncture, three days afterwards. Black blood obtained from this point showed no bacteria, either microscopically or upon cultivation.

The inferences that the writer draws are, that the ingestion of the cholera microbes produces no unpleasant symptoms; that their hypodermic injection will produce local symptoms if in sufficiently large dose; and that the blood of man and animals under normal conditions will destroy cultures of the bacteria of choleraic diarrhoea.

A RECENT JAPANESE EARTHQUAKE.[1]

AN unusually great earthquake was felt in and about Tokio on Oct. 15, 1884. The annexed autographic record of it comes, with the following particulars, from my former assistant, Mr. K. Sekiya, who is now in charge of the seismological observatory of the University of Tokio. It was given by a horizontal pendulum seismograph of the kind recently described in *Science* (iv. 516), and it has many features in common with the examples of records shown on p. 517 of the same volume. But in the present case the amplitude of the earth's horizontal movement far exceeds any thing that has been recorded since observations of this kind were instituted, in 1880.



The figure shows the record reduced to about one-third its actual size. The undulations on the inner circle have been traced by a pointer which registered the north to south component of motion, and those on the other circle by another pointer, which registered east to west motion. The pointers are prolongations of horizontal pendulums, and trace their records on a revolving sheet of smoked glass, which in this example was started into motion by the earthquake itself, through the agency of a delicate electric contact-maker. The plate is driven by a clockwork train, which, after starting, quickly reaches a steady rate under the control of a fluid friction governor. The speed of rotation was one revolution in eighty-two seconds. The short radial lines mark seconds during the first part of the disturbance. The record on the outer, or east to west, circle, has been turned round so as to bring it into synchronism with the inner, or north to south, record; and the earliest motions are distinguished in the cut by the use of a somewhat heavy line. The records begin at *a* and *á*, and are traced in the direction of the arrow, which is opposite to the direction of motion of the glass plate. At *b* the east to west record comes to an abrupt stop, owing to the displacement there having been so great as to carry that pointer off the plate altogether. The inner record extends over nearly four complete revolutions, showing that visible motions of the ground lasted for about five minutes. During the first half-dozen seconds, while both components were being registered, there is a tolerably close agreement of phase between the two, showing that the displacements were then not very far from rectilinear. The greatest motion in this part of the disturbance took place five seconds from the start. At that point the actual motion of the ground was 3.7 centimetres from east to west, and 2.2 centimetres from south to north. The displacement of the ground is multiplied four times in the original record, or about one and a third times in the reduced copy given here. The two components taken together represent a movement of the ground, from one side to the other, of no less than 4.3 *centimetres*, — a quantity which is in striking contrast to the '5 or even 7 *millimetres*' which, after three years' experience, I named as the amplitude to which in a Yedo earthquake the displacement from the mean position 'occasionally rises.'

So far as can be judged from the north to south component alone, the most violent motions were over in about ten seconds; but for some minutes afterwards, the oscillations, though very much re-

[1] From *Nature*, April 23.

duced, continued to exceed in amplitude almost any that I have recorded.

Fortunately, however, this earthquake was prevented from being excessively destructive by the unusual slowness of the oscillations. The period of the principal movements appears to have been not far short of two seconds. For a rough estimate of the greatest velocity and acceleration, we may treat the 4.3 centimetres movement as simply harmonic; and we find for the greatest velocity 6.8 centimetres per second, and, for the greatest acceleration, 21 centimetres per second, or $\frac{1}{47}$ of g. If the amplitude of motion which was recorded here had occurred in conjunction with the more usual period of three-quarters of a second or so, the destruction would have been immense. The earthquake appears to have been felt over an area of about twenty thousand square miles.

Mr. Sekiya writes, "We are going to exhibit your seismograph in the exhibition in London, to be held next May. I am sure we will get a first-prize medal."

Whether Mr. Sekiya and the Tokio university authorities get their medal or not, they should at least excite admiration for the zeal and success with which they are pursuing the study of seismology.

J. A. Ewing.

University college, Dundee.

ELECTRIC LIGHTING ON SHIPBOARD.

A paper recently presented to the British institution of civil engineers by Mr. Andrew Jamieson gave rise to an exceedingly interesting and instructive discussion. The author of the paper considered the advantages of the electric light on board ship to be summed up in the following points: its healthfulness; freedom from heat, odor, or gaseous products; its general agreeableness; its freedom from danger of setting fire to combustible material; removal of the danger of storage of inflammable illuminants; avoidance of the nuisance of cleaning and refilling lamps; reduction of space occupied by total plant; and a fair competition in cost of illumination.

The dynamo should be placed with its axis in the fore and aft line, in order to reduce the gyrostatic effect caused by rolling, and thus to lessen the heating of its bearings. It should be capable of developing the required electromotive force at its regular speed; should be self-regulating; should not 'spark;' should not heat the conductors when running light; should contain, either in its own coils or in the conducting system, not less than ninety-six per cent pure copper; and the system should have an insulation resistance of not less than ten thousand ohms per volt, generated at the regular speed of working. The speed is generally preferred to be under six hundred or six hundred and fifty revolutions per minute. Higher speeds demand more careful supervision, give rise to danger of heated bearings and sometimes of bursting the armature, cause objectionable gyrostatic action in uneasy ships, and make it difficult to drive by direct connection.

The engine should be capable of driving continuously and indefinitely as to time, without danger of heating or break-down. Its governor should control the speed within five per cent,[1] with variation of steam-pressure of ten pounds or more per square inch, and a variation of load of ninety per cent, i.e., with full load, or with nothing on but the dynamo. A tachometer, or continuous speed-indicator, is a valuable adjunct to the engine as exhibiting all variations of speed. An electrical governor acting upon the throttle-valve is thought to be a desirable instrument.

When not driven directly, the dynamo is, as a rule, connected to its engine by cotton rope, the steel-wire coiled belting coming into use in the United States not apparently having been introduced into Great Britain. The Westinghouse engine is reported to be doing excellent work. Brotherhood's 'three-cylinder engines,' and the Tower 'spherical engine,' are also working satisfactorily. Friction pulleys have been used, in some cases, instead of belting, for indirect connection.

The system of distribution is usually one of two principal kinds: in the one method, a set of return wires is used; in the other, the hull of the ship takes the return currents. The latter system is the less costly and more easily fitted, and gives rise to less resistance: but it has the disadvantages that a fault in the leading wire has more effect than in the other, a contact with the hull short-circuiting the current; it is more likely to be injured by leakage of salt water upon the conductor, in which event corrosion goes on with serious rapidity; but care in protecting the wires, and in placing them, reduces the danger from these causes to a very small quantity.

It is of great importance that the junctions of wires should be very carefully and thoroughly soldered; and the size of wire should be such that it should give at least a square centimetre area per fifty ampères, according to the rule of Sir William Thomson. But the author of the paper would adopt the rule: Make the conductivity of the wire not less than ninety-five per cent that of pure copper, and give it a cross-section of a square millimetre for an ampère and a half of current, or about a square inch to a thousand ampères; the insulation resistance of the whole circuit, including switches, etc., to be not less than a thousand ohms per volt of electromotive force of the dynamo. Failures are usually due to neglect of the precaution of testing the insulation when the plant is put in place. Safety-wires, to prevent the overheating of any part in case of wires crossing, should always be introduced.

The size of lamp should be ten-candle power for staterooms or 'cabins,' twenty-candle power for the saloons and larger rooms, and fifty to a hundred candle power for above-deck illumination. Arc-lamps of ten thousand to twenty thousand candle power are used on men-of-war for illuminating the surroundings of the ship, and for protection against the unobserved approach of torpedoes.

[1] In the United States, a variation of two per cent is considered too great.

TEN KATE'S EXPLORATIONS IN WESTERN AMERICA.

AFTER devoting himself for a number of years to the special study of somatology, and also to the more general acquirements necessary for an ethnologist, Dr. H. Ten Kate, jun., born in La Hague, set out on different expeditions to study some tribes and nations *in natura*. He accompanied Prince Bonaparte to Lapland and Finland in the summer of 1884, and in the present year has undertaken an extensive trip to Surinam, aided by subsidies from a number of scientific societies. The expedition which he undertook through the south-west of the United States and the north-west of Mexico was performed in the space of about thirteen months, — from November, 1882, to December, 1883, — and resulted in many valuable discoveries and observations, described by him at length in his recent volume written in Dutch, 'Reizen en onderzoekingen in Noord-Amerika' (Leiden, *Brill*, 1885, 464 p., 8°), with map and two plates containing views, portraits of Indians, etc. Having reached the west through Texas and Arizona, he first paid a visit to Sonora, and the southern extremity of the Californian peninsula. He found there graves of the Pericú Indians, whose skulls and bones proved them to belong to a race anthropologically distinct from the Cochimi, and other tribes farther north in the same peninsula. He left that dry country to pass through Sonora again, and north to the Gila River, to the Mohave reserve on Colorado River, to central Arizona, to the Pápago and Apache-Tinné settlements in the same territory, to Zuñi and the Pueblos of New Mexico scattered along the Rio Grande. The aboriginal tribes last seen by him were the southern Utes and the tribes in the centre and the east of the Indian Territory. We easily understand that the space of thirteen months was but a short lapse of time, considering the immense area travelled over, and the large number of Indian tribes and other objects of ethnologic interest which came under his observation; but, in reading the long and interesting report, we must acknowledge that the traveller has made the best use of the opportunities offered him. There is an endless variety of remarks on botany, geology, zoölogy; on Indian dresses, customs, pictographs, color adjectives; on government, politics, history, and political economy of the countries visited. We also meet at times with a few pungent remarks on the traders, cowboys, politicians, and 'judges' in the far west, — observations which greatly help to brighten the narrative, and enhance the interest we take in it.

Several smaller articles auxiliary to this report were issued by Dr. Ten Kate before its appearance. Their purpose is to give scientific accounts of Indian craniums, bodily admeasurements, tribal names, etc.: they are written in French.

THE GLACIAL PERIOD IN AUSTRALIA.

IN a paper on this subject recently read to the Geological society of London, Dr. R. von Lendenfeld said, that, although several previous writers have suggested that bowlders and gravels found in different parts of Australia are of glacial origin, the evidence is vague, and no clear proof of glaciation has been brought forward. During a recent ascent of the highest ranges in Australia, — parts of the Australian Alps, — where he discovered a peak which he named Mount Clarke, 7,256 feet high, he found traces of glaciation in the form of *roches moutonnées* throughout an area of about a hundred square miles. The best preserved of the ice-worn surfaces were found in a valley named by the author the Wilkinson valley, running from north-east to south-west, immediately south of Müller's Peak and the Abbot Range. No traces of ice-action were found at less than 5,800 feet above the sea. The rocks showing ice-action are all granite; and the fact that the surfaces have been polished by glaciers is said to be proved by the great size of such surfaces, by their occurrence on spurs and projecting points, by many of them being worn down to the same general level, and by their not coinciding in direction with the joints that traverse the rock. Dr. von Lendenfeld's paper closed with a comparison of the evidence of glacial action in Australia with that in New Zealand.

Prof. T. G. Bonney considered that more evidence was necessary in order to establish the point contended for by Dr. von Lendenfeld. All his proofs were founded on granite, which had a constant tendency to form rounded bosses. The fact that the supposed *roches moutonnées* occurred on spurs, rendered the matter still more doubtful, seeing that in small glaciated tracts such surfaces were chiefly found in valleys. It was a remarkable, and to him a very suspicious, fact that no moraines or perched blocks were noticed: in fact, the only point of importance adduced in favor of the author's view seemed to be the difference in the direction of the joint-planes and of the rounded surfaces; and this he thought insufficient.

Mr. W. T. Blanford agreed with Professor Bonney, and mentioned examples of the occurrence in the plains of India, where glaciation was out of the question, of granite surfaces simulating *roches moutonnées*, and of larger dimensions than those cited by the author. It seemed to him not impossible that Dr. von Lendenfeld was right; but the evidence brought forward was certainly not sufficient. The circumstance most in favor of a glacial origin for the supposed *roches moutonnées* was their restriction to a particular elevation.

THE RECENT CHOLERA CONFERENCE IN BERLIN.

AT the recent cholera conference in Berlin, May 2–8, the principal disputants were Koch and Pettenkofer, the former asserting, and the latter denying, the specific character of the so-called 'comma' bacillus of cholera.

The following summary of the position of each, taken from reports that have just reached us, may be of interest to our readers.

Koch's grounds for his assertions he sums up as follows: 1. The constant occurrence of the bacillus

in seventy-nine cases of cholera examined in Calcutta. 2. He demonstrated pure cultures of the comma bacillus from France, Italy, and Germany, all exactly alike. 3. He considers it proven that this comma bacillus occurs only in cholera, may be differentiated from others similar to it, and is diagnostic of the disease. 4. He demonstrated inoculation experiments upon animals, as follows: —

Five cubic centimetres of a five-per-cent solution of sodic carbonate, and in twenty minutes ten cubic centimetres of meat-broth containing a pure culture of the comma bacillus, were injected into the stomach of each guinea-pig. Immediately afterwards laudanum (one centimetre for each two hundred grams weight) was injected into the abdominal cavity. This served to narcotize the animals for one-half an hour to one hour. The next day they were ill, with bristling hair, great weakness of the hind-legs and muscles of the back, and died in from one to three days. Section showed swelling of the intestinal glands, and the stomach and coecum full of an alkaline, colorless, flocculent fluid, containing almost a pure culture of the comma bacillus. This experiment was made upon eighty-five guinea-pigs.

Similar experiments were made with Finkler and Prior, and Denecke's bacillus, but in much smaller number. The results were very different, Finkler's bacillus producing putrefaction in the intestinal contents, as shown by their smell.

Therapeutic experiments upon the inoculated animals showed merely that large doses of calomel, or the use of naphthaline, would prolong the life of the animal for a day at most. The comma bacillus is easily destroyed by drying and other disinfectants, as by a one-half-per-cent solution of carbolic acid.

The observations upon man, considered by Klein and Macnamara to be of the nature of infection experiments, Koch took up again, and showed, that, of the one hundred and fifty physicians who took the 'cholera course' in Berlin, but one had cholerine, and comma bacilli were found in his dejections.

He has also found that the comma bacillus will live in well-water thirty days, in dirty canal-water seven days, twenty-four hours in the contents of a privy, three to four days in moist linen, eighty-one days in the harbor-water of Marseilles (Nicati and Rietsch), and more than one hundred and forty-four days on agar-agar. Koch has never found any *resting* form at all like the spore stage of some other bacteria.

Pettenkofer confessed himself not convinced. He said the inoculation experiments were unsatisfactory. Those made with Emmerich's short staffs at Naples and at Munich were much more so. The manner in which Koch inoculated his animals threw no light upon the subject, for only man had the disease. He cannot agree that the comma bacillus is more than a usual accompaniment of cholera. The epidemiological knowledge of cholera is to be completed by considering the comma bacillus its cause, — a difficult thing to prove, since drying kills this organism; and yet in lower Bengal a dry year is notoriously a favorable one for the disease.

The comma bacilli are found only in the intestines, not in the organs; and yet the intestinal glands are highly absorptive. Cholera is not a combination of infection and intoxication, but an infectious disease, pure and simple. It is possible that in the future Emmerich's staffs may be found to be the cause of the disease. These are found in the organs of the inoculated animals, and produce cholera-like vomiting and diarrhoea. Before fully accepting the bacillus, more must be known of the epidemiology of the disease. Since cholera is not communicated directly, so the cholera-germ is not; and, since cholera depends upon place and time, the cholera-germ must be governed in the same way.

ROLLESTON'S LIFE AND WORK.

Rolleston's worthiest memorials are the growing school of biology at Oxford, and the important zoölogical and anthropological collections of its university museum. His remarkable energy, however, enabled him not only to do his work as a teacher, and take the part of a leader in university politics, but to add to knowledge by investigations in many subjects. His original papers, dealing with topics pertaining to anatomy, physiology, zoölogy, archeology, and anthropology, are scattered over the pages of different journals, and the reports and transactions of various societies. It is well that some of his friends have collected these scattered writings, and secured their republication in the volumes before us. Professor Turner of Edinburgh has edited them; and Prof. E. B. Tylor of Oxford has added a brief biography, which is full of interest as giving a clew to the source of the remarkable influence which Rolleston was able to exert in favor of natural science, at a time when the traditions, and the preponderance of the sentiment of his university, were against such studies.

Rolleston's father, vicar and chief land-owner of a small Yorkshire parish, was a good classical scholar, and undertook the primary education of his son, who, it is said, was able to translate Homer at sight when only ten years of age. The lad had, from the first, something of the tastes and instincts of the naturalist: he read Izaak Walton, and Gilbert White's ever-charming 'Selborne,' and in his play-hours mounted the skeletons of mice and weasels, and stuffed the skins of birds and beasts of the neighborhood. After subsequent years at school, he won a classical scholarship at Pembroke college, Oxford, and began residence in

Scientific addresses and papers. By George Rolleston, M.D., F.R.S., Linacre professor of anatomy and physiology, and fellow of Merton college. 2 vols. Oxford, *Clarendon press*, 1884. 76+947 p., portr., illustr., 5 pl. 8°.

his eighteenth year. He seems to have been unusually boyish for his age. A contemporary records "how young he was in every way, beginning at first sight to tell with schoolboy frankness all about his study at Sheffield, how he furnished it, how the boy next him had died, and how he had read all his Greek plays." The master of Pembroke did not trouble himself concerning the unsophistication of his new scholar. He said, "He is a clever Yorkshireman; and, when a Yorkshire-man is clever, he *is* clever."

In 1850 Rolleston graduated with a first class in classics, and next year he was elected a fellow of his college. His fellowship was only tenable on the condition that he should graduate in medicine within a certain period. Oxford affording at that time even less opportunity than now for medical studies, he went to St. Bartholomew's hospital in London. After completing his professional course he worked with notable success for a time in the English hospital at Smyrna, towards the close of the Crimean war. In 1857 we find him settled in London, and assistant physician to the hospital for sick children. Extracts from letters written at this period show him entirely devoted to his work, and interested not merely in his little patients, but endeavoring to promote the welfare of their parents. "I see a good deal of the London poor by this means, and, though I find among them much stupidity and brutishness, I nevertheless see more of qualities which are estimable. Love and self-denial I see constantly, and I make it my business to encourage these qualities."

Rolleston's career was not, however, to be that of a successful London physician. His character, his talents, and his learning were not forgotten at Oxford; nor had he lost his love for his university. Before he had practised a year in London, the Lee's readership in anatomy, and the post of assistant physician to the Radcliffe infirmary, fell vacant, and Rolleston was elected to both. For some time after returning to his *alma mater*, he was hampered in the performance of his teaching duties by the necessity of practising medicine to make a sufficient income; but in 1860, being then in his thirty-first year, he was elected to the Linacre professorship of anatomy and physiology, just endowed by Merton college. This position he held for the rest of his life. Once freed from the cares and distractions of a physician's life, Rolleston's future career was that of an earnest teacher and investigator, and protagonist in the weary war which biology had to wage in Oxford, year after year, before it could obtain any standing in the university less galling than a begrudged and contemptuous tolerance.

When Rolleston was appointed Linacre professor, the Oxford museum was being organized against much opposition, partly on financial grounds, but mainly because a powerful group of university leaders had aroused the sentiment that natural phenomena should only be studied from an artistic or emotional standpoint. The beliefs of this group were, that there was something degrading, if not absolutely obscene, in the study of the bare facts of anatomy and physiology; that skulls of early races of mankind were disagreeable objects, which no well-bred person would ever look at but through the semi-translucent atmosphere of history and poetry; that organic nature could never interest any one possessing refined feelings, except when a hazy glamour had been thrown around it by the discoloration and distortion of naked facts by mental spectacles of 'sweetness and light;' that the objective study of the question, how man came to live, and move, and have his being, was not only irreligious (which might be pardoned), but ungentlemanly, and therefore inexcusable. The forces and feelings against which Rolleston had to contend, are hard to picture in imagination now, but they were then very real and vigorous. Ten years after the foundation of the Linacre professorship, an Oxford man told us that all the natural-science students in Oxford called themselves mathematicians; and even mathematicians were regarded with contempt by the average undergraduate, whose boyish aestheticism led him to gently coo that 'culture' was all in all, and literature its only road. Against this sentiment Rolleston had to work. He had personally experienced that a student who intended to adopt the profession of medicine was heavily handicapped if he gave up three or four years of his life, after leaving school, to the sole study of Greek and Latin. As one excelling in classical scholarship, and skilled 'in all the learning of the Egyptians,' he could command a respectful hearing, even from the most conservative supporters of the eighteenth century Oxford curriculum. His indisputable excellence as a scholar, his eloquence, his energy, his executive ability, his genial nature, his universally recognized honesty of purpose, and hatred of all sham or subterfuge, enabled him to do what, perhaps, no other man of his time could have done; namely, obtain at Oxford a tolerably fair recognition of the value and importance of biological study.

No one can bring about a great reform, unless in a social medium already somewhat prepared for it. It was Rolleston's good fortune to work at a time when his efforts were not mere hopeless assaults on a fortress rendered impregnable by prejudice. He battled at an epoch when many sympathized with him, and were ready to help. But it is his glory to have been the leader, exceptionally fitted by natural gifts and academic career, to conduct to victory those who desired to widen the range of Oxford studies. To him, more than to any other one man, is it due that in biological teaching the university on the Isis is now pressing close on the heels of her sister on the Cam.

PROFESSOR MARSH ON THE DINOCERATA.

Of late years Professor Marsh has been following the plan of selecting a certain group of extinct vertebrates, and thoroughly elucidating its structure in an exhaustive monograph. Where practicable, this plan is by far the most satisfactory method of dealing with the subject; but it seldom falls to the lot of a paleontologist to obtain his materials in the necessary abundance. The volume before us is a magnificent one, surpassing in many respects all other paleontological works. Never before has such a remarkably perfect series of mammalian fossils, illustrating a single group, been brought together. Only in the tertiary lake-deposits of western America could such a collection have been formed; but few can realize what an expenditure of time, labor, skill, and money, even under the most favorable circumstances, is represented by the raw material of this work. Had Professor Marsh done nothing beyond collecting, he would still be entitled to the lasting gratitude of all biologists.

The introduction gives a short but sufficient account of the geology of south-western Wyoming, the only region where remains of the Dinocerata have been found. The section illustrating this part is open to serious criticism, in that it substitutes for the long-established names of formations given by Hayden, King, and Powell, new terms derived from some characteristic fossil. Such arbitrary changing of accepted names can only result in 'confusion worse confounded.' This section refers the Laramie to the cretaceous, whereas it is almost certainly tertiary. The Puerco is altogether omitted.

The Dinocerata: a monograph of an extinct order of gigantic mammals. By Othniel Charles Marsh. U. S. geological survey. Monogr. Vol. x. *Washington*, 1884. 237 p., 56 pl.

The descriptive part of the book opens with a chapter on the skull, in which the most curious part of these most curious animals is illustrated with much care. A remarkable and novel feature of this chapter is the series of sections of the skull which it presents. These sections are made in all directions, — transverse, vertical, and horizontal, — and thoroughly display the internal structure of the skull, the sinuses, cranial cavity, olfactory chambers, as well as the characters of those bones which cannot be seen from the surface. Professor Marsh has here indicated a new method of investigation, which is certain to yield valuable results in the future, as it already has in his hands. Strange to say, the description of the skull ignores almost entirely the basi-occipital, sphenoidal, and periotic regions, as well as the foramina at the base of the cranium. These are most important features, and their omission detracts materially from the value of the chapter. The lower jaw receives very thorough description and illustration: its chief peculiarities are the backward projecting condyles, and, in the males, the anterior flanges, for the protection of the great upper tusks. Professor Marsh shows that in the females these tusks were very small, and that in consequence the flanges of the mandible are absent or rudimentary; thus correcting the very natural error into which Speir and Osborn had fallen in regarding the flange as a generic instead of a sexual character.

The chapter on the teeth need not detain us except to notice the lower incisors and canines. Osborn and Speir first showed that these teeth differed from those of all other ungulates in having bilobed crowns. In his restoration of 'Tinoceras' and elsewhere, Professor Marsh represents these teeth as having a very different shape, though the only actual specimen he figures (woodcut 38, p. 37) is an evidently much-worn canine; of 'Dinoceras,' he gives figures of three isolated incisors. We must believe that the restoration of these teeth in 'Tinoceras' is erroneous.

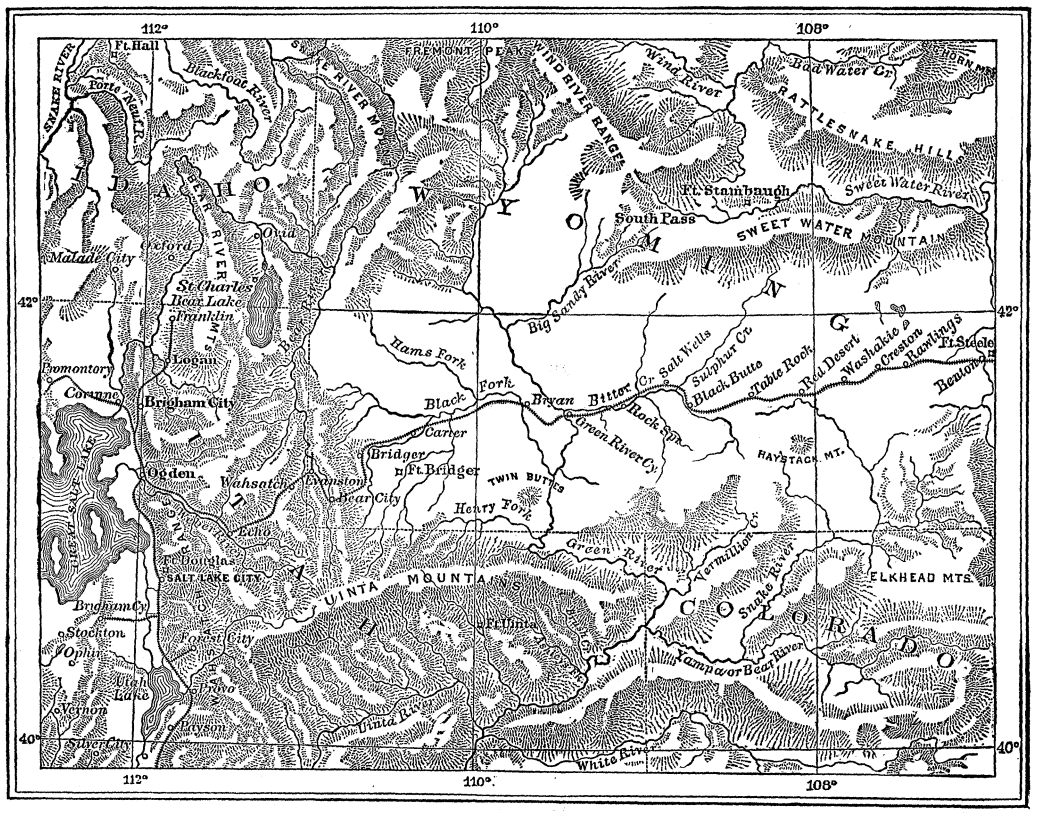
Certainly one of the most striking and valuable chapters in the book is that on the brain. The brain in the Dinocerata "was proportionately smaller than in any other known mammal, recent or fossil, and even less than in some reptiles. It was indeed the most reptilian brain in any known mammal." This is a most remarkable and unexpected fact. This chapter is enriched by an extended and valuable series of cranial casts of mammals from nearly all the tertiary formations. Lartet first pointed out the comparatively small size of the brain in the

tertiary mammals; and his results have been confirmed by Cope, Bruce, and others, but by no one with such a wealth of illustration as by Professor Marsh. The latter's generalizations, however, are somewhat vague, and not altogether novel, and in one case inaccurate. Professor Marsh says, 'All tertiary mammals had small brains.' While such is the general law, it has conspicuous exceptions; as in the lemur Anaptomorphus, described and figured by Cope, and in some miocene mammals. Of the latter Professor Marsh's selection is not the best to bring out the facts. Aside from this, Professor Marsh's assemblage of facts is of the utmost importance, and well worthy of careful study.

Chapters v.–xii. are taken up with full and accurate descriptions of the trunk, neck, and limbs. No point of importance is left in doubt, and we may be said to know the osteology of the Dinocerata almost as fully as that of any recent group of mammals. Such completeness of material, and fulness of detail, constantly excite the reader's admiration.

Chapter xiii. deals with the restoration of the Dinocerata, and will be read with great interest; and the two superb folding plates which illustrate this chapter have never been approached in the general accuracy of the separate parts, or in the beauty of drawing. In the figure of Dinoceras, however, the humerus is incorrectly drawn, and we believe that the fore-limb is too much flexed (compare plate 28, fig. 2).

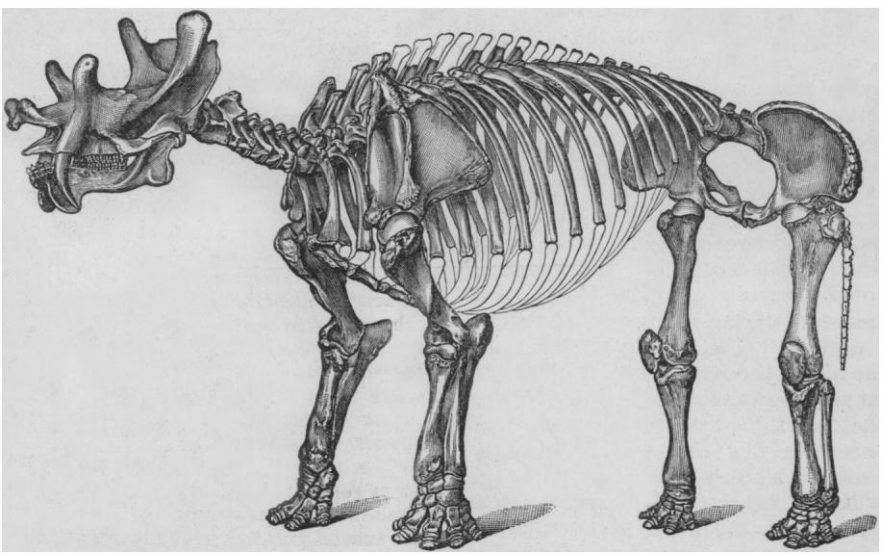
The general conclusions form the least satisfactory section of the work. Lack of space forbids any full analysis of this chapter, but some portions of it demand notice. In the main, the scheme of classification of the ungulates here proposed agrees quite closely with that made by Professor Cope (*Proc. Amer. phil. soc.*, 1882, pp. 435–447), though with some manifest improvements. No acknowledgment of this agreement is made, however, and the reader would not suppose that Cope had ever written on the subject. When the latter proposed the order Amblypoda, including Coryphodon and the Dinocerata, Professor Marsh rejected it in these words: "A careful consideration of the characters of Coryphodon, so far



as now known, indicates that the genus represents a distinct family of perissodactyl ungulates, the Coryphodontidae. The skull is clearly of this type, and the skeleton and feet present no differences sufficiently important to justify a separation from that natural order" (*Amer. journ. sc. and arts*, 3d ser. vol. xiv. p. 84). Yet in the present volume he adopts the order under the name of Amblydactyla. But the proposed new terms, Amblydactyla, Coryphodontia, Holodactyla, and Clinodactyla, are all synonymes of earlier names, and cannot be adopted. This volume is, we believe, unique among modern scientific works in not containing a single reference in the text to the work of others, and the reader never knows how much of the book has already been anticipated. There is, it is true, a scrupulously exhaustive bibliography appended; but, as few can plod through such a mass of pamphlets, injustice cannot be avoided by this method.

In conclusion, a few words as to the classification of the Dinocerata. The genus first to be named was the Uintatherium of Leidy: the Tinoceras and Dinoceras of Marsh, and the Loxolophodon and Eobasileus of Cope, were described at later dates. As far as the evidence in this volume goes, these names all refer to the same genus, which, of course, must be called Uintatherium. The shortness of this article will not allow us to attempt to prove this proposition, but we believe it capable of satisfactory demonstration. It is, however, a matter of slight importance.

The plates of this volume are beyond all praise. They are drawn with the utmost fidelity, and at the same time are most beautiful specimens of artistic skill. In this respect they may challenge comparison with any similar work. The printing and type leave nothing to be desired, and the numerous finely executed woodcuts add much to the clearness of the text. Notwithstanding, then, all that we have found to criticise, 'The Dinocerata' is a splendid piece of work, which is an honor to American scientific enterprise.



RESTORATION OF TINOCERAS INGENS MARSH. ONE-THIRTIETH NATURAL SIZE.

REPORT OF THE U. S. ENTOMOLOGIST FOR 1884.

Workers in economic entomology look forward with especial interest to the appearance of the annual report of the U. S. entomologist. The bureau under his charge is the only institution devoted to this department of science, which is liberally supported; and therefore it is rightly expected that this report shall be the most important contribution to applied entomology during the year.

The report before us, contained in the report of the department of agriculture for 1884, consists of a hundred and thirty-four pages, illustrated by ten plates. The more important articles in the body of the report treat of kerosene emulsions, the streaked cottonwood leaf-beetle, the southern buffalo gnat, and the cranberry-fruit worm. There are appended to the main report several reports by special agents.

The article of most general interest is that

treating of kerosene emulsions; and we are glad to see considerable space devoted to 'words of caution and advice' as to the dangers attending their use: for it can hardly be said that the discussions of the kerosene question in these reports have been heretofore conducted "in the spirit of an investigator, and not in the spirit of an advocate."

The experiments with kerosene, and the invention of devices for applying insecticides, have been the characteristic features of the work of the bureau during the past four years. This work has been of great importance; but it is hard to see on what grounds the late commissioner of agriculture claimed that "the chief remedies and insecticide appliances now quite generally employed with satisfaction, and constantly discussed and recommended in the agricultural press, have originated during my administration of the department" (p. 13).

The successful introduction of Apanteles glomeratus, a parasite of the imported cabbage-worm, is one of the most practical results of the work of the bureau; and the working-out of the life-history of the cranberry-fruit worm is also important. The article by Mr. Hubbard, on the rust of the orange, is very complete, except that nothing is said to lead the reader to think that any thing has ever been published before concerning this disease. This is the more surprising; since we find, that, although the mite which is supposed to cause the rust is carefully figured, the name given to it by Ashmead five years ago is nowhere used in the report. The creature is referred to as simply 'the rust-mite,' or as 'the mite.'

The illustrations are not so good as we have learned to expect in these reports. Of the figures on the ten plates, nearly one-half are reproductions, and the original figures are nearly all photo-engravings. The photo-engraving processes are a great boon to impecunious investigators who cannot afford to employ engravers; but in a small report, which is almost the only visible result of the expenditure of a vast sum of money, we have a right to look for something better. It is due to the artist, however, to say that the new figures bear inherent evidence of truthfulness.

In looking at the report as a whole, we find much in it of value, but still not so much as might fairly be expected when we consider the large number of entomologists employed (we think, fifteen), and the size of the appropriation made to the bureau (nearly $30,000 for the year ending June, 1884). It is true that the entomologist complains that the work of the bureau has outgrown its present means of putting results before the public; but this complaint would have more force if he were more economical of the space at his disposal. If the bureau has accumulated large additions to knowledge which are of great interest to the agriculturists of the country, why devote what is more than one-fourth of the report to an article on cabbage-insects, the greater part of which is a compilation from sources which are easy of access? or why devote seven pages to republishing an address on 'General truths in applied entomology'?

NOTES AND NEWS.

AT a meeting of the American society for psychical research held in Boston, June 4, a report was made by the committee on thought-transference which covered a discussion of the results of the experiments upon guessing digits and the colors of cards, which were described in a circular issued by the society during the winter. A large number of returns were received, but no evidence was obtained of the existence of thought-transference among ordinary persons for such matters as the value of a digit or the color of a card. Prof. E. C. Pickering of the Harvard-college observatory also presented a discussion of the observations taken at the observatory in the revision of the star catalogues, — observations in which it was supposed that some thought-transference might take place, as the recorder knew the magnitude of each star as given in the Durchmusterung before he received the observer's estimate. If thought-transference existed, this fact might have an influence upon the observer's mind; but no evidence of this influence was found in a discussion of some ten thousand observations. One of the members of the society has met with some success in the reproduction of drawings after the plan of the English society. The committee on mediumistic phenomena made a brief report, stating that they had visited a number of mediums, and had arranged several private *séances* on their own terms, but had met with nothing satisfactory; they will, of course, continue their work, as will the other committees of the society.

— Reports are received from the Pacific coast of unusual damage by insects destructive to crops. Locusts, presumably Camnula pellucida, are just now very destructive in the unfledged condition in some ten counties of California, especially in the San-Joaquin valley. The genuine Hessian-fly is also doing much damage to the grain districts embraced in a line drawn from Vallejo in Solano county to Benecia, thence to Suisun, thence to Napa City, and back to Vallejo; also in parts of Sonoma county.

— The *Athenaeum* states that the Russian traveller Piassetsky, who accompanied Col. Sosnoffsky on his journey through China and Mongolia in 1872, and a translation of whose travels was published last year by Messrs. Chapman & Hall, is about to set out on

a second journey to China. The Russian papers announce that he recently showed the drawings and paintings, made during his earlier tour, to the emperor and empress, who expressed themselves much interested in the prospects of his second journey of exploration. Piassetsky owed his escape from several unpleasant predicaments, during his former travels through the Middle Kingdom, to his skill as a draughtsman; and it is hoped by his compatriots that he will be able to turn this advantage to better account, now that he can follow his own course without the interference of a superior officer like Col. Sosnoffsky, with whom, on the last occasion, he continually disagreed.

— At the annual meeting of the Iron and steel institute in London, Mr. Andrew Carnegie of Pittsburgh contributed a paper on natural gas-fuel and its application to manufacturing purposes, the information being much the same as that contained in his recent article in *Macmillan*. In the discussion which followed, Mr. J. H. Bell said there was a possibility of Cleveland, Eng., competing with Pennsylvania in the matter of natural gas, as in other matters, as the salt-beds evolved a certain quantity of gas, and, if the borings were continued several hundred feet farther, would probably give more. At the last day's meeting, Dr. Herman Wedding of Berlin contributed a paper on the properties of malleable iron, in which he said that microscopical investigation had led him to modify the explanation of welding he had given some years ago. He had now come to the conclusion that the strength of a finished piece of iron depends on the sectional area of the mass of iron it contains. From the total sectional area of a piece of weld iron, the slag inclusions, and in the case of ingot iron the blow-holes, must be deducted. This calculation is decidedly in favor of the ingot iron, though he pointed out it can only be superficially effected, even with our present knowledge of microscopy.

— A valuable illustrated note on the Huron (Dakota) tornado of Aug. 28, 1884, has just been issued by the signal-service: it is the work of Sergeant S. W. Glenn of the signal-corps. Imagine, the author says, a vast treeless plain, void of hill or dale, and a sultry atmosphere beneath a sky unobscured save by small drifting cumulus clouds and a narrow band of stratus cloud in the north-east. Suddenly a commotion is observed in the cumulus clouds which have piled up in a woolly mass in the north, as though checked by some invisible barrier separating them from the horizon by a strip of clear sky. Then there is a rapid and confused whirling: the centre of the mass drops down bowl-shaped, and appears as if making futile efforts to touch the earth. At the same time, a conical cloud of dust is seen to accumulate on the ground, and acquire a rotary motion. With the swiftness of thought, the upper cloud drops a considerable distance downward, and spins out a white, ribbon-like line towards the ground. The connection between the earth and cloud being established, it remains stationary for a moment, apparently gathering strength before starting on its career of destruction. Then it moves rapidly over the plain, destroying every thing in its path. A number of cattle and horses were taken from a herd, lifted bodily high in the air, and churned together in a living mass; thirty steers and four horses were killed, and more were wounded; most of the beasts appeared to have their lower jaws dislocated. The tornado crossed the Dakota River, taking up the water so suddenly as to leave the bottom exposed for an instant: the water was carried to a great height, and was not seen to fall; but heavy rain, with some hail, occurred twelve or more miles to one side of the track. Mr. Glenn is of the opinion that the centre of the tornado was a nearly complete vacuum, and that the governing factor of the storm was electricity; but he does not state how electricity could produce or maintain the vacuum, and he gives too brief consideration to the dynamic effects of the whirling air at the centre of the tornado funnel.

— A party of five persons — consisting of Drs. E. G. Gardiner and H. M. Buck of Boston; Mr. G. H. Barton, assistant at the Massachusetts institute of technology; and Messrs. Bartlett and Burlingham of the junior class of the institute — has just started on a summer's expedition, under Prof. A. Hyatt, to the west coast of Newfoundland for zoölogical and geological explorations. The party sailed in the Arethusa, a small schooner belonging to Professor Hyatt, which he has used in connection with the summer school at Annisquam. Professor Hyatt left some weeks ago for St. John, N.F., where he has been visiting the museum, and securing charts and a pilot. He will join the party at Cape Breton.

— During the years 1870 to 1879, the meridian circle of the Harvard college observatory was largely employed in the revision of the Durchmusterung for the zone between the parallels of declination at +50° and +55°. The star-places employed as points of reference in the work were taken from the list given in publication xiv. of the *Astronomische gesellschaft*, and various stars were also observed for purposes not connected with the revision proper. In a quarto pamphlet of nearly one hundred pages, and extracted from the forthcoming volume xv. of the annals of the observatory, Prof. W. A. Rogers, who has himself made nearly all of the observations, and has had charge of their reduction and publication, makes the results of the entire work immediately available in the form of a catalogue of the right ascensions and declinations of 1,213 stars. The catalogue proper is preceded by the annual results for the fundamental stars, while the data of the catalogue itself are derived from a discussion of the results obtained during the whole period covered by the observations.

— M. K. Olzewski communicated to the French Académie des sciences, on the 6th of April, a paper on the liquefaction and solidification of formine and of nitric acid. Cailletet stated that he had first made known the procedure for the liquefaction of these gases and their use in a condensed form for obtaining the liquefaction of oxygen. Olzewski, by preparing products free from acetone and hydrogen, has succeeded in obtaining a white, snow-like mass.